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SOME PRACTICAL ASPECTS OF THE PLAGUE AT SYDNEY.

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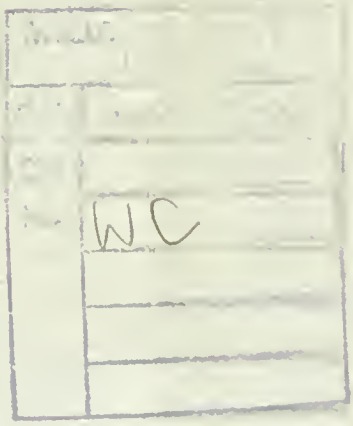

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SOME PRACTICAL ASPECTS OF THE PLAGUE AT SYDNEY.

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(Member).

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In responding to the honour of the Committee's invitation to address you on the subject of the plague at Sydney, I am not prepared to present a connected account of that epidemic. The official report, which will soon be available to you, is much more complete and satisfactory than anything I could reproduce from memory. I have to ask your acceptance of a paper unavoidably sketchy, as owing to the lack of means of reference to precise data and documents, I am obliged to offer you broad outlines rather than detailed pictures. But this is perhaps not much to be regretted since classical descriptions of plague have now become so numerous, that it is little short of an infliction to have to read, let alone write, another. With this claim upon your indulgence, I venture to invite your attention to some items of our experience in Sydney as regards, (1) the recognition of plague, (2) its mode of dissemination, and (3) the preventive measures applied.

(I.) The Recognition of Plague.

The incidence of plague on Sydney was to some extent anticipated. That is to say, its wide dispersal during the last few years, and especially its southerly trend, led us to prepare ourselves, as far as possible, to meet and cope with it should it

appear amongst us. But so far as the recognition of the disease was concerned, we had to depend on the mental picture we could form by perusal and analysis of available descriptions: for none of us had had any practical acquaintance with plague, nor was there in our medical community any gentleman so equipped to whom we could look for aid. It thus came about that, for my own part, I awaited the expected crisis with all the doubts incidental to untested conceptions. As a matter of fact the first case was a perfectly clear and straightforward one, and thus fortunately was recognised without difficulty. But our subsequent experience was to the effect that whilst the majority of our cases were equally easy of diagnosis, we would now and then meet with one concerning which we could express no decided opinion on the clinical evidence presented. Speaking generally, one may say that given a case exhibiting symptoms common to several acute febrile diseases, the only clinical manifestation that obviously invests it with the specific character of plague is the existence of a bubo. In the absence of this indication one can rarely be more than suspicious that the case is plague; the final adjudication, if forthcoming at all, is expressed by the bacteriologist, or only delivered at the *post-mortem* table. Hence it appeared to me that the points of most practical importance in the recognition of plague were associated with (*a*) the characters of buboes, (*b*) the indications other than buboes which should induce a suspicion of plague, and (*c*) the means of obtaining a bacteriological examination. I have therefore selected these three matters for consideration in this part of my paper.

Buboes.—I am unable to state from memory the exact proportion of bubonic cases in our epidemic, but that type of the disease was exhibited by the great majority of our patients. We had one or perhaps two pneumonic, and the rest were septicæmic. But I may here observe that although the same type was usually preserved throughout the illness, this was not always the case. Bubonic patients commonly became septicæmic and showed bacilli in the blood, and sometimes there was a secondary pneumonia, with bacilli in the sputum. Such mutations cannot be predicted, nor can they always be detected. I call attention to them in this place merely as an indication that it is not wise to attach a too rigid interpretation to the terms used in classifying the cases in an epidemic.

Our experience was not altogether in accordance with the statement that buboes usually appear on the third day of illness. We generally found them present on the second, and in many cases on the first day, whilst in a few instances they were amongst the very earliest symptoms noticed by the patient.

I recall upon this point the instance of a man, who first felt sick about noon, and developed a painful swelling in his groin about 4 p.m. on the same day. When I saw him next day at 2 o'clock—twenty-six hours after his falling sick—the bubo was already as large as a pigeon's egg, and formed a visible prominence in the left femoral region. In another case the first indication of illness was a sharp pain due to a bubo in the groin suddenly experienced whilst the patient was walking along the street. From our own cases I should have inferred that the bubo usually made its appearance before the third day of illness.

We had a preponderance of femoral buboes, although again I cannot state the exact proportion, nor the relative numbers on the two sides. It was most usual to find the lowest gland of the femoral group enlarged either alone, or to a greater extent than the others, but, as might be anticipated from anatomical considerations, this was not invariably the case. I am not quite sure whether inguinal or axillary buboes were next in order of frequency, but I think there was not much difference between the numbers. It was not common to find an inguinal bubo alone; there was generally some associated swelling of the adjacent femoral glands. In the axillary region, the bubo was frequently situated in a true gland of the armpit. One had to feel well up in order to palpate it. But there was a fair number of cases in which the bubo occurred in a gland under the outer edge of the pectoral muscle, and was not therefore a true axillary bubo. It happened on one occasion early in our epidemic that a bubo in this situation was overlooked at the first examination. It was then very small and did not happen to be pressed upon during palpation of the axilla. It revealed itself later, but the incident taught us to carefully palpate with slight pressure all glandular regions, whether indicated or not by the patient as a site of discomfort or pain. I may add that in a sick cat forwarded to the laboratory for examination and found to be suffering from plague, such a sub-pectoral gland was the site of the bubo. Cervical buboes were not very common, and when they occurred generally affected the glands in front of the sterno-mastoid muscle. We did not meet with any instance of popliteal bubo, and although in one case there was a swelling of the epitrochlear glands, specimens taken from it for bacteriological examination were repeatedly examined with negative results.

In speaking of buboes in the foregoing remarks, I would be understood as referring to the primary glandular swelling. As a rule indeed there was only one, or merely local extension

from one group of glands to another in its neighbourhood. I have already mentioned the association of inguinal and femoral swellings, but in addition to this it was very common in purely femoral cases to elicit tenderness on pressure above Poupart's ligament. The indication thus afforded of extension to the iliac glands received confirmation by *post mortem* and experimental observations. But apart from such cases, enlarged glands were occasionally present in two totally different regions, as groin and axilla, or axilla and neck. They were sometimes on the same, and sometimes on different sides of the body. In a few instances there were bilatual enlargements, always either femoral or inguinal or a mixture of both, but the symmetry was never quite perfect—one side being more advanced than the other. I cannot say that the subsequent development of the second swelling, or of either of them, was in any way affected by the existence of the other.

As a general rule the buboes appeared quite suddenly, and enlarged rapidly. In the course of twenty-four hours the glands sometimes attained the size of a pigeon's egg, or even that of a walnut. But on the other hand, occasionally they never became bigger than a marble. It was usual to find several glands of a group enlarged, but one much more than the others. The detection of the gland itself, however, depended on very early access to the case. If this were obtained it was usually easy enough, by the rounded or oval shape, well-defined limits, &c., to satisfy oneself of the existence of a swollen gland. But it was not many hours before this became impossible, owing to the occurrence of hæmorrhagic exudation and œdema round the gland. When this had happened all that could be felt, as a rule, was a diffuse thickening in the region, having no definite shape nor margin. The amount of exudation, like the degree of swelling of the gland itself, was very variable. Sometimes it formed a mass filling up and causing a projection in the groin or axilla, and at others it was so scanty as to be scarcely evident. When the gland itself could be felt it was tense rather than hard, its consistence being, to my mind, comparable to that of the testicle. The periadenitic effusion was more or less firm, at all events at first, but later on it sometimes became soft enough to take an imprint of the fingers.

The skin over the bubo remained normal in colour for a time, and then became reddened, whilst with the approach of suppuration it assumed a more violet tint. Sometimes in the later stages it showed discoloration as in a bruise, and in one or two instances there was actual hæmorrhage of a petechial character.

In nearly all cases the bubo was very tender; in some cases

exceptionally so—the patient flinching and guarding it with hi hand, or begging that it might not be touched. But usnally gentle manipulation could be borne, and sometimes the swelling could be pretty freely handled without causing any great amount of pain. Apart from handling, the sensation in a bubo was described as a dull ache rather than actual pain, but this was often severe enough to induce frequent requests for renewal of hot applications, which seemed to be of great comfort to the patients. Any movement involving pressure on the bubo caused acute pain, and qniescent patients sometimes assumed positions which would minimise such pressure. Those with groin buboes would flex their thighs at the hip; those with axillary buboe hold their arm away from the side, and those with cervical bnboes keep their head rigidly bent towards the side on which the bubo was situated. In a few instances the bubo appeared to be painless whilst the patient remained quiet. It may be that an impression of painlessness was often due to the patient not being able to appreciate his own sensations owing to mental depresssion or aberration.

The ultimate fate of buboes included resolution, suppuration, and sloughing and ulceration,—the exact issue depending upon the stage at which the inflammatory process stopped. Resolution was sometimes remarkably rapid, all sign of the swelling disappearing in a fortnight or three weeks from its first appearance. It was more common, however, for the signs of active inflammation to subside within this period, and the swelling to remain as a practically insensitive lump, gradually diminishing in size during the course of one or two months. The buboes that suppurated were opened in the usual way, and usually contained sanquinolent or creamy pus; occasionally shreds of necrotic tissue. After opening, the discharge became thinner and serous in character. Very often the sinus healed up in the ordinary manner, but sometimes its edges broke down and left a ragged ulcer, which ultimately went through the slow process of granulation.

It resulted from these various modes of fermination, that whereas some patients were quite well and able to leave the hospital within three weeks of their admission to it, there were others who had to be detained for nearly as many months. This was a practical point in the management of the isolation hospital, as the accommodation became strained now and again, owing to the accumulation of such convalescents. We had some doubt as to the proper way of dealing with that class in which the buboes, though inactive, nevertheless persisted as lumps. Our hand was forced to some extent by the pressure on our accommodation, and after observing the first few cases,

we reluctantly had to let the rest go as soon as we were satisfied that inflammation in the buboes had really ceased. Of course, they were also completely free from constitutional symptoms. There is no theoretical reason really opposed to this procedure, and nothing against it appeared in our practice. It seemed to be a perfectly safe thing to do. But the question of patients with open sores was another matter. They were perfectly well except for the local lesion, which was sometimes very small. But although our own observations were in accord with the statement that plague bacilli cannot be detected in the discharges after a certain time, we did not feel justified in releasing such patients whilst any sign of discharge remained. Consequently, they accumulated in the hospital, blocked up beds, filled the convalescent wards, and so impeded the proper reception of acute cases. They thus constituted a real practical difficulty for which we discovered no solution.

Indications of plague other than buboes.—Failing the detection of a bubo, the opinion one can form amounts to a more or less well-grounded suspicion, based as far as I was concerned on general indications rather than precise symptoms. I do not wish to imply any neglect to ascertain, and estimate the significance of, the state of the temperature, pulse, tongue, etc., but that in plague, as in most other diseases, the search for characteristic physical signs was practically consequent on the impression afforded by the general aspect of the case. I have therefore elected to endeavour to portray the general conditions in which we found our various patients, adopting this course the more readily since the detailed symptoms exhibited by them were in accord with the many excellent descriptions now available, and with which you are no doubt familiar.

I call attention in the first place to cases in which the incidence was so swift, and the virulence so great, that the only suspicious element was the sudden death of the patient, and the only means of verification the *post-mortem* examination of the body. Thus there was one case in which a man, who had got up, taken his breakfast and gone out in accordance with his usual custom, was drinking at a bar when he complained of feeling very giddy and sick. He remained depressed and miserable for some time, and then went to the back premises of the hotel. A little while afterwards he was found dead in an outhouse. It was only at the autopsy that he was ascertained to have been stricken down by plague. There was another case in which a young girl fell sick one afternoon with headache, vomiting, etc., her attack being such as commonly follows, and was actually ascribed to, some dietary indiscretion. She died next day before noon, and on examination

her organs generally were found to be teeming with plague bacilli. I may also mention that in another case in which the illness was recognised, the interval between the first symptom and death was only eighteen hours. These illustrations serve to show that our epidemic was not wanting in fulminant manifestations, the plague toxin occasionally exhibiting a potency little, if at all, less than that possessed by some of the vegetable alkaloids and mineral poisons.

Next in intensity to those already quoted came cases in which the incidence was equally rapid, but the virulence of less extreme degree. Amongst these were many instances of men becoming prostrated whilst at their work, to which they had gone in the morning without the least suspicion of being otherwise than in their ordinary state of health. For example, a printer had gone to business and worked at his machine for several hours. About mid-day he was seized with violent headache, nausea, and abdominal pain of a colicy character, and in a few minutes was stretched, pallid, tremulous, and prostrate, on the floor of the workshop. About three hours afterwards, when we saw him, he was lying upon some sacking with blanched face and closed eyes, helpless, motionless, speechless, in a condition of torpor from which he could not be roused. He was sent directly from the workshop to the quarantine hospital, and next day developed a bubo in his groin. This degree of prostration, though not often reached so quickly, was comparatively common in cases after admission to the hospital. Absolute unconsciousness was unusual, save as the immediate precursor of death, but a condition of profound apathy was very frequently seen. The patient would generally lie curled up in bed, on one or other side, but sometimes assumed the position of dorsal decubitus. There was no rigidity about either position, the patients could be easily moved into any other, and sometimes changed on their own accord. But having taken up such a position they would remain in it for hours, lying quietly with their eyes closed, perfectly indifferent to their surroundings, content so long as they were left alone. If disturbed they would wearily open their eyes for a moment and bestow upon one a vacant glance, perhaps frightened, perhaps reproachful, and promptly relapse into their torpid condition. They would swallow liquid food and medicine poured into their mouths, submitting to this and other necessary attentions without apparently the least appreciation of what was being done to them. Even the handling of the bubo usually failed to produce more than a wincing of the face expressive of pain, unaccompanied by any movement of resistance. This condition sometimes ran on to coma and death, and sometimes passed

away in a few days, leaving the patient very weak but convalescent.

The next type of illness to which I direct attention was that exhibited by the majority of our patients. The onset, though still rapid, was not so appallingly sudden as in those already mentioned. There was an interval of some hours between the premonitary symptoms and prostration, during which the patients were usually able to get home to bed. The very first case of the epidemic occurred in a carter who had gone to work in the morning feeling perfectly well, as was usual with him. About twelve o'clock, whilst driving his team, he suddenly developed violent headache, giddiness, and nausea, which compelled him to tie up his horses and lie down. He could not eat his lunch and continued to feel sick, but he managed to resume his journey and completed it about four o'clock. At that time he felt a pain in his groin and on examination found a lump there. He reported himself sick, put up his horses and went home to bed. He is said to have vomited and been very sleepy and feverish during the evening. He was visited next day at noon by his private medical attendant, and in response to a report by that gentleman I saw the patient at two o'clock. He was then curled up in bed as if sound asleep, but he roused at once when I spoke to him. His face was pale, with a febrile flush on the cheeks, and his expression that of a man not quite conscious of his whereabouts. He answered my questions in a weary kind of way, but quite intelligently, and although speaking slowly the enunciation of words was perfectly clear. He professed feeling very weak, comparing his condition in this respect to those experienced in a previous attack of influenza. He could easily move about the bed and sat up without assistance when requested to do so, but he evidently preferred the recumbent posture, resuming it as soon as possible. For the rest I need only mention that he had a temperature of 105·4°, a rapid but fairly strong pulse, and a left femoral bubo as large as a pigeon's egg.

The picture here presented, though a common one, was often modified by a much greater degree of torpidity. The patients could be less easily roused from their sleep-like condition, and on being left alone promptly relapsed into it. When so lying in bed the expression on their faces sometimes suggested dreaming, and whilst at others placid, it was never entirely vacant. When roused up the expression was often one of anxiety, fright, or bewilderment, as when a person is awakened from deep sleep. They would answer questions, or attempt to do so, often exhibiting the clipped or mumbling speech mentioned by authors. Their drowsiness often resulted in their not following

what was said to them, and in their failure to utter more than half sentences, forgetting the rest. They were generally bidable, being too sick to resent or resist interference, and moreover they could generally be made to appreciate the efforts made for their comfort. The positions assumed were also indicative of the retention of sensibility, for in these patients one observed the attitudes designed to relieve pressure on a bubo, viz., flexion of the thigh, elevation of the arm, &c. The condition was of variable duration, and of no significance as to the ultimate issue of the case.

In other instances, although the onset itself was not excessively rapid, there was a period at which the symptoms would suddenly become distressingly acute. For example, a young man having spent the evening socially at a friend's house, went home and retired to bed, apparently in his usual state of health. During the night he vomited, and next morning complained of nausea and headache. He did not go to business, and appeared to get better during the day. Towards evening he became listless, drowsy, and light-headed, and when seen at midnight he was flushed with fever, quite prostrate, and delirious. He was transferred to the Quarantine Hospital early next morning, and in that institution had a series of hæmorrhages from the mouth, nose, and bowels, developed a purpuric rash, and died on the fifth day of illness during copious bleeding from the lungs. It was not often that patients were found delirious at an early stage, this condition more commonly supervening after admission to the hospital. The delirium was generally noisy, taking in some cases the form of incessant chattering, and in others that of more or less frequent spasmodic outcries. In nearly all cases the utterances were incoherent, babbling, and meaningless, with perhaps a single word distinguishable here and there. Associated with the mental, there was also muscular unrest, tremors, spasmodic contractions, or purposeless movements of the limbs or head. Rarely a patient would excitedly sit up in his bed, or try to get out of it, and sometimes struggle when his efforts were resisted.

In connection with the class of case now under consideration I may perhaps be permitted to cite one illustrative of the mode of attack in a young child. A little boy, aged two years, had been bright during the day, but was noticed to be unusually quiet in the evening. During the night he had a series of convulsions. Next morning the child was admitted to one of the general hospitals. There he lay all day in his cot, very pale, drowsy to torpidity, having no regard for food, nor the surroundings, and making no sound unless disturbed, when he uttered plaintive cries. This condition, for which there was

no discoverable cause, roused the suspicions of the superintendent, and in our joint examination of the case next day we discovered a bubo in the axilla, fluid obtained from which contained an abundance of plague bacilli. The child died early next morning.

The series of cases just quoted represent conditions sufficiently serious to arrest attention, and lead to further investigation. Hence they usually came under our notice at an early stage, and presented no special difficulties in the matter of diagnosis. But interspersed amongst them were many which failed to suggest the idea of plague, by reason of the mildness of the symptoms at the time they were examined. In these cases the onset was sometimes comparatively slow, and the amount of enfeeblement moderate. Suspicion attached to them mainly on account of their unexplainable illness having occurred during the prevalence of a plague epidemic. Such patients would sometimes be found in bed, complaining of the general aches and weariness that so commonly precede the onset of a fever, and often indicated the lumbar region of the back as the site of their greatest intensity. The recumbent attitude, however, was more a matter of inclination and comfort than necessity. The invalidism was of a mild character, and the patients could, and did, attend to their own wants, getting out of bed actively enough when they had occasion to do so. Frequently they were not found in bed, but up and dressed, sitting moodily in a chair, or moping about the house in a restless, aimless kind of way. Their mental condition was usually quite clear. They frequently had no definite symptoms, showing little beyond slight elevation of the temperature, with corresponding increase in the frequency of the pulse, and perhaps a coated tongue, anorexia, constipation, etc., such as might attend the onset of many febrile illnesses. Consequently it sometimes happened that judgment on such cases had to be suspended for a time, but it was usually not long before their true nature was revealed by the appearance of a bubo, or the accession of severely prostrating developments. As an illustration of the class of case here referred to I recall the instance of a clerk who went to business one morning, being, as far as he knew, in his usual health. He had an attack of colic and vomiting during the day, and went home early. He attended his office next morning, and although seedy, remained there all day. Next morning he went to work again, but as he looked ill and complained of weariness, headache, and aching of the body and limbs, he was allowed to go home. It was next day—the fourth of his illness, that I was asked to see him by his private medical attendant. I found him up and

dressed, wandering about his house, and looking, as he expressed it, "a bit knocked out." He had some pallor, a pinched expression, walked slowly, and let himself down into his chair in a way suggesting feebleness of old age. The wife said he had been "talking a lot of nonsense," but he spoke to me intelligently, and knowing my name exhibited considerable curiosity as to my opinion of him. He was also alert enough to refrain from mentioning a swelling under his arm, where I subsequently found a bubo as big as a walnut, and surrounded by diffuse œdema. He went to the Quarantine Hospital in the afternoon, and died three days afterwards.

As in this patient, so in others of the same class, there was no real clouding of the intellect. They were sometimes irritable, or peevish, and resented interference, but quite alive to the objects of our visits, and perfectly prepared to mislead us if they could, as to the real nature of their illness. For as the epidemic progressed we became known as the plague inspectors, and our clinical attentions carried with them the prospect of incarceration at the Quarantine Station. This was not always conducive to cordiality of reception, nor to that perfect confidence which should exist between doctor and patient. It was particularly in the matter of buboes that fertility of explanation was most manifested. They were ascribed to what was commonly called, "barking" of the skin, to scratches with dirty metal, to cuts, or abrasions, to anything, in fact, that would tend to divert suspicion from their real cause.

On the other hand there were many patients, representing the mildest type of the disease, who, because they had a bubo or some other symptoms described in that morning's paper, voluntarily went to the hospitals, or came to the offices of the Department of Public Health, in order to confirm or dissipate their own impression that they were attacked by plague. These people were often worried by the anxiety of doubt, and our diagnosis, whichever way it went, was usually a great relief to them. It is not to be supposed that those who really had plague represented true ambulant cases; they usually became very ill later on and some of them died. They are mentioned as showing that there is not inevitably any marked reduction of strength at a time when the case nevertheless can be clearly recognised as plague. It may indeed be asserted that in a few cases there was never anything approaching serious debility throughout the whole period of illness.

I may mention finally upon this point that the condition presented at any particular time could rarely be relied upon as a guide to the future progress of the illness. At nearly all stages up to established convalescence there was a liability to sudden

and unexpected changes of serious or even fatal consequence. As will be explained presently the patients were always conveyed in charge of trained attendants from their homes to the Quarantine Hospital, first by ambulance and then by steam launches. There was one man transferred from his house to the ambulance, and from the ambulance to launch upon a stretcher, who felt so little ill that he refused to obey the nurse's order to remain lying down. He sat up during the half hour's journey down the harbour and appeared to be enjoying the trip. As the launch approached the landing jetty, this man stood up, waved his arm, fell back, and died almost immediately afterwards. No doubt his death was due to failure of the heart consequent on the degeneration of its fibres that occurs in plague. It was particularly in patients apparently mildly ill or progressing favourably that this untoward complication was apt to occur with startling abruptness, and we learned to regard the issue of any case as uncertain until all constitutional symptoms had quite passed away.

In the foregoing paragraphs I have dealt with the question of the clinical recognition of plague from the health officer's point of view, devoting attention to the subsequent course of the illness only in so far as was desirable in order to complete the illustrations. The endeavour has been to represent, so to speak, the various grades of illness in which our various patients were discovered when first coming under our notice. In doing this I have ventured to offer illustrations not only of cases of typical or average severity, but also of others expressive of variations up to the extremes in both directions. The general outcome is to suggest that plague can be recognised if there be a characteristic bubo, and reasonably suspected on evidence of a person having been suddenly and violently stricken down with illness of a severely toxic nature. Where neither of these conditions obtain, a decision must perforce be withheld pending further investigation and developments. I would submit, however, that the effort to present a comprehensive view has involved the elimination of minor details to which one or other clinician has attached considerable diagnostic significance. But the value of these is to be appreciated only or mainly by those who have had practical acquaintance with plague. Their accurate interpretation depends on what it has become fashionable to call personal equation; they constitute the bright spots of that kind of experience that has been compared to the stern lights of a ship, illuminating only the track that has been traversed. However valuable to their possessor they can rarely be conveyed to others, and therefore I have abandoned the idea of attempting to delineate such delicate indices.

The means of obtaining a bacteriological diagnosis.—The third practical point in the recognition of plague selected for consideration concerns the means used to obtain a bacteriological examination. The arrangements made at Sydney were, that on report from a private practitioner of a case of plague, or one that might be such, it was immediately visited by a member of the official staff of the Department of Public Health. The visiting medical officers carried with them in their bags, amongst other things, a sterilised hypodermic syringe fitted with platino-iridium needles, a platinum wire loop in a glass handle, two culture-tubes, one agar and one serum, a metal box of sterilised cotton wool, a small bottle of lysol, a small wide-mouthed stout glass bottle, a spirit lamp, and about a dozen cover-glasses. These articles were all prepared beforehand in the laboratory of the Department, and as used were replaced by a fresh set. With this apparatus they were always able to obtain, in doubtful cases, samples of bubo fluid, blood, or sputum, in a way that gave a reasonable prospect of success in the subsequent examination.

We never attempted to incise a bubo, save in the hospital, on account of suppuration; the fluid for diagnostic examination being always obtained by puncture and aspiration with the needles and syringe, after preliminary cleansing of the skin. Any fluid obtained was conveyed to the laboratory in the barrel of the syringe, and there taken from it to inoculate culture tubes and a guinea-pig, and to make cover-glass preparations. Sputum was taken in the sterilised wide-mouthed bottle, and at the laboratory treated in the same way as bubo fluid. With blood, the culture tubes were inoculated, and cover-glass smears prepared at the bedside of the patient.

Puncture of a bubo did not always yield fluid for examination. In the early stage success depended on puncture of the swollen gland itself, which had to be defined and steadied between the thumb and fingers of the left hand whilst performing the small operation. In the later stages this was less important, as the exudation round the gland contained abundance of plague bacilli. The amount of fluid obtained was rarely more than a few drops, and in some instances, when none appeared in the barrel of the syringe, one could still obtain enough from the channel of the needle. It was also possible, in some cases, to express a drop or two of fluid out through the needle track by gently squeezing the bubo, and with the small quantity so obtained to make cultures and smear preparations as in the case of blood. The fluid obtained at these examinations was seldom purulent, but usually a greyish coloured mixture of lymph and blood, containing innumerable plague bacilli. Our

experience was similar to that of other observers, in that bacilli were relatively scanty in pus from suppurating buboes. Specimens of blood rarely showed plague bacilli, save when taken from patients who died within twenty-four or forty-eight hours afterwards, and even in these the micro-organisms were usually few, and discovered only after long search. The cultures from blood were more useful so far as recovery of bacilli was concerned, but the patient was generally dead by the time one could examine and pronounce upon the growth. It afforded confirmatory rather than diagnostic assistance, and this applies also to guinea-pigs inoculated from the small clots often found on the culture tubes inoculated with the blood. I have nothing special to remark with regard to sputum, as very few samples came under my notice.

I need not in this place enter upon any detailed account of the laboratory operations and their results. Our observations were in accord with the authoritative descriptions, and are fully recorded in the official report. Suffice it to say, therefore, that they usually afforded valuable assistance, but occasionally failed to throw any light upon the case, owing to practical difficulties in the way of obtaining suitable specimens. A bacteriological diagnosis was specially serviceable to us at the beginning, to determine individual cases, and establish the existence of the epidemic. Later on the cases were clinically diagnosed with increased confidence, and a bacteriological examination required only in doubtful or important instances. Our experience of it was that with care it did not mislead, but we had to recognise limitations to its practical utility.

(II.) The Mode of Dissemination.

With reference to the epidemiological aspect I can only in this place present a brief summary of the evidence obtained concerning the origin and mode of spread of the plague amongst us at Sydney, and again refer you to the official report for details.

Our first patient fell sick on January 19th, 1900; the second about four weeks later; the third about four days after the second; and then followed the rapid succession of cases comprising the epidemic. We ascertained no reason to doubt that the case reported in January was actually the first that occurred in Sydney. Examination of the monthly mortality statistics for January, 1900, December and November, 1899, showed no essential differences between them and the data for corresponding months of previous years. Enquiries at the hospitals, and of medical men with large practices afforded no indication of

the existence of any excess of patients with buboes (ambulant cases) during the months in question. I have no doubt that any such circumstance would have been noticed, since medical men were fully alive to the possibility of meeting with plague, partly on account of the prevalence of that disease at Noumea (four days journey by steamer) and partly because of the comment aroused amongst them by the reported occurrence of a case at Adelaide. But neither at the time nor since has any member of the profession asserted his cognisance of a previous case in Sydney, and we did not succeed in gleaning even a suspicion that any person sick with plague had come to the Colony from abroad. Consequently, although experiences elsewhere have been such as to induce caution in expressing assurance as to the primary incidence, all the evidence we could gather was uncompromisingly to the effect that the first person to acquire plague was the man reported to the Department of Public Health in January.

The fact that this first patient really had plague was vouched for by clinical and bacteriological evidence of such a convincing description as to place the diagnosis beyond doubt But we were quite at a loss as to the manner in which he had become infected. He had never been away from the Colony, and careful investigation failed to reveal any receipt or handling of possibly contaminated articles. The one suggestive fact in this respect was that he was a carter in the employ of a shipping firm, and his business took him to certain wharves which were the scene of the next incident in the epidemic, and in other respects a very important feature of it.

Immediately upon the occurrence of the first case a proclamation was issued, in the official Gazette, in the public press, and by printed placards posted up in all parts of the city. requesting information with regard to sickness or unusual mortality amongst rats. It was not many days before we were advised by a customs officer that the rats were dying in great numbers upon one of the wharves, and on the afternoon of the same day on which this report was received we obtained fifteen rats from the wharf—some dead, some sick, and some apparently healthy. *Post-mortem* and bacteorological examinations showed that no less than seven of them had plague. We immediately set men to work to trap and kill the rats, and many lots of them were examined in the laboratory with sometimes positive, sometimes negative results. Enquiries made of government officials employed about the wharves—customs officers, fruit and grain inspectors and others, elicited the information that the dead bodies of rats to an unusual amount had been noticed for some weeks before our attention was

called to the matter. The epizootic amongst the rats was thus indicated to have commenced at a date prior to the occurrence of the first case in a human being. As already stated this patient had been upon some of the wharves in the vicinity of that upon which the plague rats were discovered.

The next episode was the death from plague—bacteriologically confirmed—of a man who resided immediately opposite this very wharf, whilst the third patient was a clerk employed actually upon it. The series of cases which next followed, with a remarkable exception to be mentioned directly, were all in persons who either lived in, or frequented, the vicinity of the wharf in question. The exception referred to was that of a family living miles away from the wharf, and in which, within two or three days of one another, the father and three children fell sick with plague. They were very poor people, and occupied a dirty dwelling swarming with vermin, situated adjacent to the municipal garbage tip. The children were said to be in the habit of playing upon the tip, and the other members of the family were also believed to have dealings with it. At this tip there was deposited the rubbish collected, amongst other places, at the wharf already mentioned, and the presumption is that the infection of this particular family was acquired from some article, rat, or vermin carried thence by the scavenger's carts. So that these cases also were associable with the wharf and its plague rats, and its locality remained for a period of several weeks the only one from which infection appeared to be derived.

Of course the people becoming infected in this area fell sick and were found by us in various other parts of the city and suburbs, but it was some time before what may be called indigenous cases arose in any other place. There was a spread continuously along the water front, and for some little distance inland into the city; but it was not till later that cases appeared in persons residing outside the area of connected extension, and having no relationship with it. Such cases occurred in women and children, or in men who had not been away from their district within a time covered by the period of incubation of plague. These separate areas in which plague thus appeared to obtain, as it were, an indigenous foothold, were not specially those at which persons infected at the primary (wharf) focus had fallen sick. Very often the indigenous cases were the first or only ones that occurred in them. The one fact indicative of the possible mode of importation of plague into such areas was that in nearly every one of them we sooner or later captured rats, which on examination at the laboratory were found to be infected with plague. They were

PLATE 1.

PLAGUE RAT NATURALLY INFECTED, SHOWING GREAT ENLARGEMENT OF THE SPLEEN AND LIVER.

PLATE 2.

GUINEA PIG DEAD AFTER INOCULATION WITH PLAGUE (PARTLY DISSECTED, LIVER AND ALIMENTARY TRACT REMOVED) SHOWING MILIARY NECROTIC SPOTS IN SPLEEN AND HÆMORRHAGES IN LUNGS AND HEART.

PLATE 3.

GUINEA PIG DEAD AFTER INOCULATION, DISSECTED TO SHOW SWELLING OF THE SUPRARENAL BODIES AND HÆMORRHAGE IN KIDNEYS AND BUBO IN RIGHT GROIN.

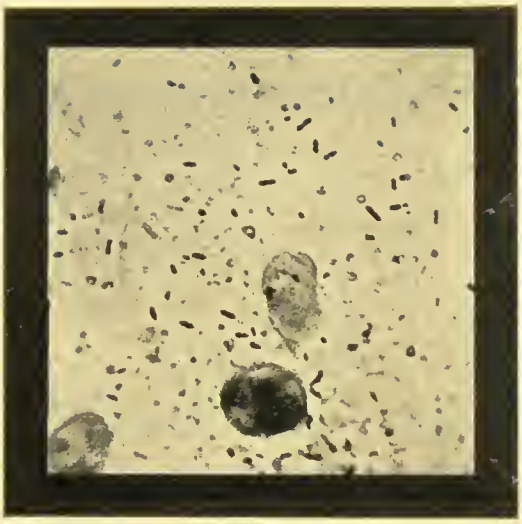

FIG. 4.
PLAGUE BACILLI IN FLUID FROM BUBO.

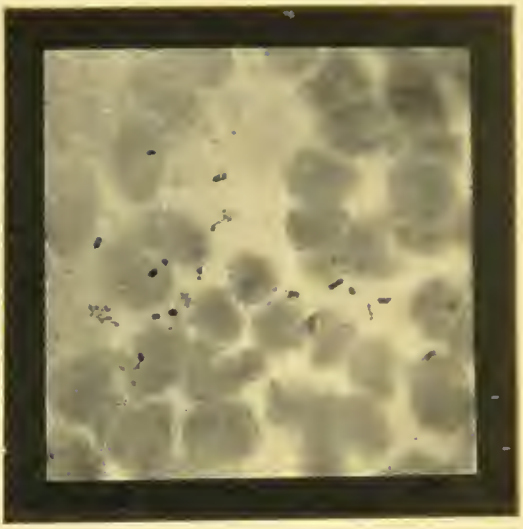

FIG. 5.
PLAGUE BACILLI IN FLUID FROM A CARBUNCLE.

FIG. 6.
PLAGUE BACILLI FROM AN AGAR CULTURE.

FIG. 7.
PLAGUE BACILLI FROM A BROTH CULTURE.

PLATE 4.

taken not only from the locality, but in a few instances from the actual houses in which plague occurred. In this and in other ways it became impressed upon us that the incidence upon human beings was practically always associated with disease of the rats. We were confronted, as others have been, with the triangular problem in the transference of plague which this dual existence of the disease involves, viz., infection from animal to animal of the same species; infection from man to rat; and infection from rat to man.

The one clear fact in our epidemic was that human beings were not becoming infected directly from one another. We had occasionally two or three cases in the one household, or amongst the employees of a single business firm, but almost invariably the attacks were either practically simultaneous or separated by such an interval as excluded the possibility of infection by direct contact. In most instances, however, there was but one case in a family or staff, and there was an entire lack of immediate relationship between it and any other of the epidemic. On the one hand the patient had not seen, nor been near, and often did not know any infected or possibly infected person, and on the other no secondary cases arose in his household or elsewhere as the result of his own illness. It very often happened that patients fell sick in remote suburbs or in country towns to which they had gone by railway; circumstances that could not fail to result in the detection of any secondary case arising from them. But neither in these, nor in any other instances, was there the least evidence that one person had acted as the source of infection for another. There was no ground for even a suspicion that our epidemic was being maintained by any process of direct contagion between man and man.

We did not observe any case in which the infection could be ascribed to handling or receipt of contaminated articles. The only evidence bearing upon this point was that some three or four, out of as many thousands of the men engaged in cleaning up the infected areas, acquired plague. They were working in different places, handling different materials, and fell sick at different times, so that they did not serve to indicate any particular articles as infectious. Moreover, as they were employed in places harbouring infected rats, not necessarily those contaminated by human beings, they were on the same footing as other patients not engaged in the cleaning operations. We have, therefore, no evidence that infection could be acquired by any process of indirect contagion. It thus came about that the disease was not observed to spread either directly or indirectly from man to man.

With regard to the natural means by which the disease was

kept going amongst the rats, we have no very definite evidence to offer. Experimentally, however, they succumbed to inoculation, and to pricks and scratches with infected instruments, such as might be received from teeth or claws in quarrels with fellow rodents. Encounters of this kind were frequent amongst rats kept in the laboratory cages, and sometimes culminated in one of the party being reduced to a few claws and a backbone. In captured rats we often found bites on the ears and nose, and if these had been received during an argument over the body of an infected confrere, they would no doubt have been the means of introducing plague into their recipients. But from our examinations this did not appear to be a common mode of infection, since we never found true buboes in captured plague rats. There is a possible fallacy in this inference, since plague is apt to assume a septicæmic form in rats, and hence there would not be a bubo. But swelling of the nearest lymphatic glands, accompanied by hæmorrhagic exudation, was usually produced by experimental inoculation, and consequently one would expect it to have been seen if the natural infection was acquired in a similar way. We found that rats could be infected by feeding, the experimental animals being kept in glass jars by themselves, and given soft material, bread or biscuit soaked in cultures of plague bacilli, or the spleen and liver of rats or guinea-pigs recently dead from plague. Feeding with the organs was much more successful than feeding upon cultures, the latter sometimes failing to produce infection. In one instance we found a large mesenteric gland in a captured rat, and in one of the experimental rats the whole group was swollen, matted together and embedded in hæmorrhagic exudation, indicating that infection had been acquired through the gastro-intestinal tract. Owing to their cannibalistic proclivities, possible injuries from bones, and the probability that a comparatively helpless sick animal would be regarded a fair object for attack, feeding may be supposed to contribute something towards the maintenance of epizootic plague in rats. We never succeeded in producing plague in rats or other experimental animals by placing them in the uncleaned cages and jars in which animals were dying or had died from plague. I may here remark that our laboratory operations were never attended by accidental conveyance of infection amongst the animals more or less closely associated in the various experimental investigations conducted by us. We risked upon two occasions the scattering of numerous fleas over an infected and sick rat, to test whether these vermin would convey the disease to a healthy one kept in the same cage, but separated by a double wire screen with wide meshes,

as mentioned by Simond. The experiment was negative on both occasions, and I am glad to say that we had no further acquaintance with the particular fleas used in the experiment. We did succeed in setting up infection by means of an emulsion of fleas taken from a very sick rat captured at a hotel from which we had just taken a girl suffering from plague. The bacilli were detected by microscopic examination of the emulsion, and injection of a few drops of this fluid into a guinea-pig produced plague fatal in the usual time; the animal showing the usual *post mortem* appearances including a bubo; and its viscera yielding micro-organisms, with sub-cultures of which we were able to produce plague in other animals. We are therefore able to make this small contribution in favour of the statement that plague bacilli may be found in or on fleas, and such vermin may thus be the means of disseminating the disease amongst rats. Briefly summarised, our experiments seemed to indicate that the disease might spread from rat to rat by inoculation through wounds from infected teeth or claws, or sharp points of bone in their food, or by vermin, and by feeding upon the viscera of infected animals. It appeared also that feeding upon infected food of other kinds, such as biscuit or bread deliberately soaked in culture would occasionally result in infection, but usually did not, and no positive result was obtained by feeding upon the food merely soiled by a sick animal.

Our epidemic does not furnish any clear evidence on the question of the transference of plague from man to rat. That the excretions of plague patients contain bacilli, and that rats feed upon such infected excreta in the sewers, suggested itself to us as a possible mode of such conveyance. To a certain degree the extension of the primary focus was along rather than across main sewer lines, and the secondary indigenous foci occurred on small reticulations connected with the larger ducts, suggesting that rats may have picked up the infection from material carried along the sewer. But this did not cover all cases, and the time relationship was not in keeping with such a theory. It appeared rather more probable that the sewers constituted highways along which infected rats migrated, and thus disseminated the disease from the primary focus. Meanwhile I need only say that we did not observe any clear facts indicating a spread from man to rat.

The last point in the problem under consideration was transference from rat to man. I have already stated that at the wharf in the neighbourhood of which the bulk of our early cases occurred, mortality amongst rats preceded the incidence of plague upon man; and that in other indigenous centres

plague rats were almost invariably discovered. Many of our patients had actually handled dead rats, others had seen them about their houses and premises, or what seemed to impress them much more strongly, they had smelt them. These facts, considered in conjunction with the entire absence of immediate or mediate contagion, induced us to believe that rats were the sources of infection for human beings. As regards the exact manner in which the microbes obtained entrance into the bodies of our patients, the evidence was to the same effect as that obtained elsewhere, namely, that they were infected by inoculation. The preponderance of femoral buboes indicated that the actual site of inoculation was usually somewhere on the lower limbs, but the suggestion of oriental epidemiologists that infection occurs with dust entering small unnoticed wounds on the bare feet did not apply to our cases, as our people all wear boots. We never had any patient who could be supposed to have received his infection through wounds—cuts or abrasions. We had therefore to fall back upon the theory promulgated by Simond and others, that fleas were responsible for the actual transference of the causal bacteria. We could nearly always find fleas upon the rats forwarded for examination. This was so commonly the case that as a measure of self-protection we adopted the practice of using chloroform to stupefy the fleas, and then burnt the fur off the rats, and so destroyed the fleas before allowing the cages to be brought into the laboratory. The number of fleas upon rats specially examined was very variable, but sometimes there were a great many upon sick rats. We had no difficulty in obtaining a dozen fleas from such a rat for the purpose of making the emulsion already mentioned. I am not prepared with the names of the species of fleas found on rats, as I knew no one in our Colony who could advise me upon that point. But some of the creatures were large, like those found on dogs: and others were small, showing on comparison no difference that I could detect from fleas obtained from human beings. It has been asserted by Galli-Valerio that fleas from rats will not bite mankind, but this has been disputed by other observers, and I can at least vouch for the fact that rat fleas promptly make for one's hands when a rat is manipulated. I need scarcely say that we did not wait for a practical demonstration as to whether they would bite. For my own part, I am inclined to believe that although fleas have a predilection for particular species of animals, they will go upon others and often sample them by a bite or two, before returning to the host best fulfilling their epicurean inclinations. It is said, and appears probable, that the fleas will remain upon rats until their par-

ticular host dies. The circulation then ceasing and the body becoming cold, the fleas are compelled to search elsewhere for their accustomed pabulum. They may probably seek another rat, but on their journey invade some other animal, perhaps man. On this point I may mention that we were informed that at the time the rats were dying in large numbers upon the wharf to which attention was called above, the fleas there were so numerous that the labourers tied strings round the bottoms of their trousers to protect themselves against the onslaughts of the vermin. Some of our patients recollected being bitten by fleas at a date consonant with an incubation period of three to five days. The majority had no such remembrance, but the social status and domestic habits of most of them were not such as to invest a little incident of that kind with remarkable novelty. They were often oblivious of having been bitten when their skin showed abundant evidence of it. In several instances we found the tiny blisters or phlyctenules mentioned by Simond and others as marking the actual site of inoculation by fleas, and in a smear preparation from one such case, we found micro-organisms having the microscopical characters of plague bacilli. On the whole, therefore, we became possessed of some facts which seemed to show that Simond's contention was not altogether hypothetical.

In view of all the evidence,—positive as regards rats and fleas, and negative as regards other agencies,—we were compelled to believe that our people were being infected from plague rats, probably through the intermediation of plague fleas or similar vermin. The liability of cats to plague, as mentioned by Lorans of Mauritius, and confirmed by the instance at Sydney, may perhaps be regarded as an additional condemnation of the rat, in view of the association between the two species of animals.

We did not succeed in discovering how the rats first became affected. The Port of Sydney is in frequent communication with infected places in China and India, and also with the Sandwich Islands and Noumea, where plague was prevalent at the time of our outbreak. But there was no evidence that any contaminated article or plague rats reached our shores from any of these places. The only fact bearing upon the point is that amongst the rats forwarded to the laboratory for examination there was a large rough black-coated rat, different from the local rats and said to be an Indian species. This particular specimen was perfectly healthy, but if it really be a foreigner, its presence in Sydney paves the way to the assumption that infected rats may have reached us by some similar means. Unfortunately, up to the present there has been no opportunity

of obtaining an authoritative opinion as to the species of rats already established in Australia, so that no special significance can be attached to the incident just mentioned.

III.—Application of Preventive Measures.

It is a curious fact, mentioned by Hirsch as regards former epidemics, and by several modern writers with respect to the present pandemic, that there has often been exhibited a tendency to deny the existence of plague on its first appearance at any particular place. It was not surprising therefore to find that the attitude assumed by our own community was at first one of manifest incredulity. The press, the public, and the medical profession, were strongly disinclined to accept our pronouncement on the first case. The bolder spirits amongst them did not hesitate to accuse the authorities of error without, however, taking the trouble to ascertain the facts for themselves. Fortunately the evidence with regard to that case was so clear and convincing that the officials of the Department of Public Health were able to maintain an unwavering adherence to their diagnosis, and to meet the storm of ridicule by firm insistance on the application of the requisite preventive measures. The discovery of the plague-stricken rats was the incentive for the organisation of a special service to cope with the epidemic thus suggested as imminent, but owing to the solidity of the passive resistance, our efforts in this direction made but slow progress at this time. For a period of about six weeks we were exposed on the one hand to adverse public criticism, and on the other to hesitancy in the acceptance of our recommendations. This undesirable condition of affairs underwent an abrupt change when six cases occurred, and terminated fatally one after the other in rapid succession. The former disbelief gave place to excited appeals for guidance. There was no longer any question of excessive zeal on our part, on the contrary we were overwhelmed by hysterical advice as to what ought to be done. With the melancholy satisfaction derivable from this long delayed removal of opposition, we set ourselves to the arduous task imposed upon us, and proceeded to a more elaborate campaign against the pestilence.

Briefly stated our procedure was as follows: On receipt of a report from a practitioner of a case of manifest or suspected plague the patient was immediately visited by a medical officer of the Department. If the case proved to be plague, the inspector informed the sergeant in charge at the nearest police station, who therenpon dispatched a constable to take charge of

the house and prevent any person from leaving it. The medical officer also telephoned from the police station to the Department, and by this means a nurse and ambulance were sent to remove the patient, and a waggonette for other persons or contacts. The patient was conveyed to the Quarantine Hospital, and the contacts to the observation enclosure within the Quarantine Station. Sometimes the removal was suspended for twenty-four hours or so pending the result of bacteriological examination of specimens taken to the laboratory of the Department by the medical officer. After removal of its occupant the house was locked up and left in charge of the police, who admitted the disinfectors and afterwards looked after it till the return of the contacts. Business premises were not closed unless at least two employees had become plague-stricken. It was then treated in the same way as a private house, but otherwise disinfection was carried on, one floor at a time, whilst business remained in progress. When an infected area became defined, a proclamation was issued giving twenty-four hours notice of suspension of business within certain prescribed limits. After that time it was surrounded by a cordon of police, and a gang of men turned in to clean up the whole area. Meanwhile efforts were made to destroy rats, by the employment of professional rat-catchers and by stimulating private enterprise to that end. Finally we carried out public inoculation with Haffkine's prophylactic obtained from Bombay. These various operations were kept going for a period of seven months. Their administration involved no slight effort on the part of the authorities, and their execution necessitated the organisation of special staffs for the different branches of the work.

Under ordinary circumstances the medical staff of the Department of Public Health were two (now three) specially qualified hygienists; but there were in addition two Medical Officers of Health, more or less closely in touch with the central office. On the outbreak of plague the medical staff was increased to seven members; two stationed at the hospital, three engaged in regular visitation of the patients, and two mainly occupied in administrative and other general matters connected with the epidemic. From time to time we had the assistance of medical visitors from other colonies who came to gain experience, and of many medical men practising in Sydney, but these were chiefly employed in connection with the inoculation of Haffkine's fluid. I must here remark that, once convinced of the reality of the plague, the medical profession generally gave courteous and loyal support to the Department.

The discovery of plague rats was accepted in the microbiological laboratory as a signal to clear the decks for action,

and when the demand came for bacteriological diagnoses and experimental observations the staff were ready and prepared to meet it. The work done in this branch of the Department was so exacting that for a period of three or four months the assistants were in the laboratory every day—Sundays, highdays and holidays included—from 8 o'clock in the morning till nearly midnight.

The general office was inundated with letters and besieged by callers, requesting advice, assistance, employment, concessions, or interviews; instituting protests or claims for compensation; seeking information concerning the sick or their effects; propounding schemes for the eradication of the plague or the treatment of the sick; and otherwise materially augmenting what may be termed the legitimate business connected with the epidemic. To meet the stupendous increase of work additions had to be made to the clerical staff in nearly every branch of the office.

The preventive measures included the removal of both patients and contacts, an action which, in the absence of statutory sanction, had to be carried out under Executive authority. For although there was ample power under the Quarantine Act and regulations to prevent a person with plague from entering the colony by sea, there was none to enable us to deal with a case arising within our own boundaries. The Public Health Act covers at present only notification, and it was our hope that in time this would furnish the means of educating the people up to the point of accepting an Isolation Act. The consequence was that we had no legal right to remove the patient, but the emergency was held to justify this course, subject to subsequent confirmation by the Executive. As a matter of fact there was never any real difficulty about the patient. The community came quite solidly to the view that he was better isolated, and resistance, though occasionally threatened, was never practised. But it was otherwise with regard to contacts. They, being quite well, naturally enough objected to being taken from their homes, and the fact that those removed invariably remained unaffected, was quickly brought forward as an argument for the abolition of our practice. For our own part we recognised that there was room for debate upon this question, and were prepared to substitute isolation within their own houses, and daily visitation for a period, in lieu of their removal. But we asked for discretionary powers in this respect, pointing out that a large isolated house was less dangerous and more easily disinfected than the small crowded dwellings occupied by the poor. This proposal was rejected as a matter of political expediency, so we had to accept

the next best thing, and remove all domieiliary eontacts indiscriminately. This procedure worked smoothly enough when it concerned five or six persons in a private house, but the evaeuation of hotels and large boarding-houses, with forty or fifty contacts, at a moment's notiee was not aceomplished without considerable strain of the transport and eommissariat. However, it was done as a matter of eourse, and as the Quarantine Station at Sydney is a delightful and picturesque plaee, the proeedure eventually came to be regarded as having provided a very enjoyable excursion for many deserving citizens.

The means of eonveyance eomprised an ambulanee and waggonette serviee, and a flotilla of steam launches. By the former the patients and eontaets were conveyed from their houses to the Quarantine Depôt and Wharf at Sydney, and by the latter they were taken about six miles down the harbour to the Maritime Quarantine Station at North Head. The ambulanees and drivers were drafted from the regular staff of the (Government) Coast Hospital at Little Bay near Sydney, and aecomodated at the Depôt, where additional quarters and stabling were erected for the purpose. Each ambulanee carried a trained nurse and the usual equipment. The steamers eomprised the regular serviee launehes and others specially ehartered, and were provided with shelters and other arrangements for the welfare of the patients. Although apparently eomplicated, the arrangement worked quite steadily when once organised.

On arrival at the Quarantine Station the patient was transferred to the permanent hospital buildings situated on a promontory and well isolated, whilst the contacts were eondueted to apartments in pavilion houses on what is known as the healthy ground. The general management of the station was under the immediate eontrol of the Superintendent of Quarantine and the permanent officers of his staff, assisted by hands temporarily employed during the epidemie. The resident hospital staff comprised two medical men, a dispenser, and the nccessary complement of trained nurses and wardsmen. All the nurses were officers of the Government Coast Hospital or Quarantinc Serviees, who, with the devotion eharaeteristie of members of their profession, spontaneously volunteered for plague duty. In addition to the resident staff the hospital was visited regularly by a senior member of the medieal profession, and frequently by the medical officers of the Department of Public Health. Facilities were afforded for medical men to visit the hospital and familiarise themselves with the disease.

The disinfecting staff eonsisted of gangs of men acting under the guidance of trained quarantine officers; the whole of this business being under the supervision of the Medical Officer of

Health for Sydney. The methods of disinfection practised comprised fumigation with sulphur or formalin; spraying and washing with corrosive sublimate or carbolic acid solution; clearing out and lime whiting of outside premises; steam disinfection of articles exposed to contamination or their destruction by fire when they were not worth preserving; and renovation where such appeared to be necessary. The Public Health Act enabled us to compel owners to repair structural defects, and its stringent application resulted in the institution of new drains and modern sanitary fittings in many places sadly in need of them. As opportunity offered, or could be made, ferry boats and coasting steamers were also dealt with by the staff here under consideration.

The proeedure in dealing with proclaimed areas consisted as stated, in a general clean up by large gangs of labourers temporarily employed for the purpose. The number so engaged averaged about 2,000 men, acting under the direction of an experieneed contractor, and the Sanitary Inspector of the Department of Publie Health and his assistants. In special cases they reeeived the advice or direction of the medical officers, engineers, and arehiteets in the Government service. The objeet sought was to make a clean sweep of the areas dealt with, removing from them everything that could be regarded as defective from a hygienic point of view. The men entered and cleaned up every street, lane, house and premises; sweeping, repairing, washing and renovating good structures, destroying old or tumbledown premises, clearing out and levelling yards, tearing up old and laying new drains, and generally doing anything of the kind that suggested itself as a means of effecting cleanliness. The combustible rubbish was burnt at bonfires in the streets, and such as could not be so treated was loaded into punts, towed off beyond the harbour, and cast away several miles out at sea. It need scarcely be said that this measure got rid of an enormous amount of rubbish, and effected material improvement in the areas dealt with one after the other, covering in the aggregate a large part of the city. Of course it was not accomplished without imposing individual hardship, but in such instances the sufferers received adequate consideration from the Government. As the cordon of police posted round the area in which eleansing operations were in progress restricted ingress to authorised persons, and incidentally prevented the residents inside from getting out, provision was made for their maintenance at the publie expense during the period of their incarceration. The interruption of business was the subject of many protests and claims, but for the most part these received little sympathy from the community, the

general impression being, by that time, that the operations represented salutary efforts towards sanitary reformation. Finally the Government resumed the greater portion of the water front, and intend replacing the present irregular wooden wharves with stone and cement structures, such as will remain cleaner and afford less harbourage for rats.

During the whole time the work of destroying rodents was stimulated by every possible means. Professional rat-catchers, real or alleged, were engaged, poison was supplied free of charge, and private efforts inspired by a reward of first twopence, then sixpence, per head, per rat, delivered at the destructor depôt. Lengths of sewers were blocked up and fumigated with sulphur, the subsequent flushing carrying down numerous carcases of rats killed by the fumes. The same method was successfully applied to the holds of ships. The use of baits soaked with cultures of bacillus typhi murium (Loeffler) or Denysz' bacillus, did not prove efficacious as rat exterminators. It is true that rats commonly disappeared for a time from places where such baits were laid. But they were not found dead, neither on the surface nor in the runs, and usually reappeared in the course of two or three weeks. On one occasion their apparent depletion at one place was coincident with great numerical increase at another about a mile distant, thus indicating that the rats had not been destroyed but had merely migrated. They afterwards gradually returned to their former haunts. It may also be mentioned that rats became so scarce upon the infected wharves as the epidemic progressed, that it was difficult and sometimes impossible to obtain specimens for examination. This illustration of the fact that these rodents will migrate from places where they are being injured or harassed, suggested the possibility that our efforts might result in the dissemination of plague by scattering infected rats. But upon reflection that sooner or later they would scatter of their own accord, as an instinctive means of ridding themselves of their epizootic, we felt justified in encouraging rather than uselessly trying to check this natural process of elimination of the disease. Besides, from our point of view, every rat killed was one possible source of infection the less, and we accordingly persisted in destroying them by every means in our power. I cannot state the number of rats that met their fate by these efforts, as the returns had not been made up when I left Sydney, and the war against them was still being carried on, but it was said that something like 100,000 had been accounted for up to that date.

The last point to which I will call attention is the public inoculation with Haffkine's prophylactic. As part of our

scheme of preparation for a possible outbreak of plague, we were in possession of a small quantity—about 300 doses—of the prophylactic when the first case occurred. This was used to inoculate the contacts with this case, and the members of the staff of the Department who would have to deal with it, and any others that might occur. At once a cable message was dispatched to India requesting a further supply, and in response to it 25,000 doses reached us about five weeks later. The same cablegram arranged for a regular monthly consignment until further notice. On receipt of the fluid, it was announced that any person could be inoculated free of charge on application at the offices of the Department. There was no great response at first, but as cases of plague continued to crop up, and the public became assured by the experience of friends or prominent persons that inoculation was not synonymous with sudden death, the number of applicants rapidly increased. In the course of a week or two the offices could not hold the crowds that collected. The corridors, staircases, and approaches were crammed with people pushing and struggling to reach the inoculators; damaging each other and smashing the furniture and fittings of the office. Members of the staff who were outside could not get in, and those who were inside could not get out; the conduction of any other business was out of the question. The affair culminated in the office being cleared by the police and the operations being transferred to a large exhibition building, with a police guard to control the assemblage. With the object of securing the utmost protection from the use of the prophylactic it was announced that only persons residing or occupied in the infected areas would be inoculated, and these had to produce written testimony of their eligibility signed by clergymen, employers, or well-known persons. For days the attendance was far greater than could be provided for, although we frequently treated twelve to fifteen hundred persons per day. This exciting episode of the epidemic continued until we had used up two consignments of prophylactic, when delay in the arrival of the third afforded us a much-needed respite. On recommencing operations the epidemic had begun to subside, the panic ceased, and the attendance gradually fell off until we had no further applications.

The actual inoculation was carried out in the following way: The back of the bared upper arm of the patient was washed with lysol solution by an attendant, the inoculating physician injected the fluid subcutaneously, and a second attendant immediately placed a small square of Mead's plaster over the puncture. The two attendants here mentioned were senior medical students, the physician a practitioner in the city whose

temporary services were retained by the Government. There were usually four to six physicians operating at a time. The instruments and prophylactic were in sole charge of the trained laboratory assistants, who attended to the preliminary sterilisation of the apparatus, poured out the fluid and filled the syringes with strict regard to bacteriological cleanliness. The syringes were provided with platino-iridium needles, which were sterilised by heating to redness in the flame of a spirit lamp between each inoculation. To the technical skill and careful observance of instructions is to be attributed the total absence of untoward results in the many thousands of persons inoculated under the exacting conditions above described.

The doses of prophylactic given were in accordance with the directions upon the bottles. The after effects were more or less fever and malaise for twenty-four or forty-eight hours, and a varying degree of cellulitis of the arm enduring for about a week or more. In some cases the febrile attack was sharp, and followed by langour for a few days, but as a rule it did not prevent attention to business. The cellulitis of the arm was sometimes painful for several days, and occasionally a small abscess formed at the site of inoculation. In three such cases that I had an opportunity of examining the small amount of pus was sterile. Beyond what is here mentioned the inoculations gave rise to no trouble, and were not attended by any serious inconvenience to the patients.

It is difficult to pronounce as to the efficacy of the process, in view of the other vigorous measures taken to subdue the epidemic. Some six inoculated persons subsequently acquired plague, but they were all mild cases and recovered. As the proportion of deaths in other persons was one in three, it is to be presumed that the previous inoculation had effected some satsifactory modification. There was in addition the advantage of its moral effect on the populace, who were considerably quietened by the feeling that they had been afforded every possible chance of protection against the disease.

From what has been said it will have been gathered that no efforts known to us were spared in combating the epidemic. The measures taken involved great expense, and an excessively arduous devotion to duty on the part of those charged with their execution. Happily we had the consolation of seeing the epidemic decline and come to an end, which I sincerely trust will prove to be a permanent one. In conclusion, I have to express my appreciation of the honour accorded to me by the Committee's request for a paper, and to thank my audience for the patient hearing of what I feel to be an inadequate recognition of it. I am above all conscious that I have oc-

cupied your attention this evening for a time unduly long, but I hope you will regard this as inseparable from a desire to place at your disposal, as fully as possible, the information acquired by us at Sydney on a subject of mutual interest and importance.

[*This discussion applies to the papers by* Dr. Frank Tidswell *and* Dr. S. M. Kaka.]

Mr. Shirley Murphy, the Chairman, said he had pleasure in asking the meeting to accord a cordial vote of thanks to Dr. Tidswell and Dr. Kaka for their interesting papers—papers which were of the greatest importance to them at the present time. We did not know what was before us in England or in London as to plague. Of course we did not anticipate for a moment anything so terrible as what had happened in Karachi, but inasmuch as Glasgow had had the plague to deal with, other parts of this country might have to do the same. They might have to develop an administration to look after it—in fact, the London County Council had already developed an administration to inquire into such cases should they occur. Few persons in London could claim to have had experience of the disease, but all could learn what had happened in India and Australia and other places where the plague had assumed prevalency, and it was very valuable to them to have the information derived from those sources, focussed in such excellent papers. He looked upon the papers as of very considerable value and worthy of detailed study.

Dr. Franklin Parsons (London), in opening the discussion, said the papers to which they had listened had been exceedingly valuable from an administrative point of view, because in dealing with such a disease as the plague they had to consider not only what modern science pointed out as practicable to counteract it, but also what measures popular feeling would accept. It was therefore of very great use to know how plague was looked upon from the point of view of the populace in countries where it had actually occurred, both in India among an alien people, and in Australia among people of like modes of looking at things as ourselves. They had to make people familiar with the mode in which the disease was propagated, with a view to obtain their co-operation. He had heard how when cholera first came into this country it was received with panic, and the doctors were accused of poisoning the patients; but in 1893 when it obtained some foothold in Grimsby there was no panic, because the means of prevention were known and were readily accepted by the people,

with the result that the disease was soon stamped out. From the admirable way in which the Glasgow authorities succeeded in stamping out the plague, he should think that the population of this country was sufficiently educated to accept any measures which might be necessary to counteract the disease should it ever make its appearance. It seemed to him that the great danger lay in the fact that so few people had seen the plague. Probably few of those present, except the two gentlemen who had read papers, had ever seen a case of plague. That was the danger, for experience had shown that when a comparitively unfamiliar disease—such as typhus or small-pox—got ahead, it had always happened because the first case had not been recognised. He noticed that both the readers of papers had laid stress upon the rats as a means of propagating plague, but he believed that the Glasgow authorities could not find any evidence to connect rats with the outbreak that occurred there.

Dr. R. T. Hewlett (London) said it had fallen to his lot to see one of the six or eight English cases of plague which had occurred, and he also had had the opportunity of examining bacteriologically three of the English cases. Many writers had called attention to the important part which rats played in the propagation of the disease, but he thought Dr. Tidswell's observations on that point were more striking than anything he had ever read. He took it that Dr. Tidswell regarded plague as having been introduced into Sydney by the rats. With regard to rat-fleas, it had of course been suggested that they conveyed the disease to man, but some observers denied that the rat-fleas would bite the human beings. He (Dr. Hewlett) was very interested to hear the amount of inoculation work which was carried out in Sydney, and possibly Dr. Tidswell might be able to give them some idea of the value of those inoculations. He would also be glad to know if the anti-plague serum was used, and if so with what result. He had recently had occasion to inquire into the value of anti-plague serum, and for that purpose had been to the Pasteur Institutes at Paris and at Lille, and at both those institutions the opinion was that there was a great future for the serum. There was one peculiar feature about the plague, and that was the manner in which it rose and disappeared. The history of plague had always shown that apparently the disease had existed in certain localities endemically, and then for some reason or other it began to spread, until at last it was an almost pandemic disease. It appeared in different localities, and without any apparent reason it disappeared, and then the world became again entirely free from plague.

The Chairman said he wished it to be distinctly understood that in recent years there had never been plague in London. It had been in the Port of London, but the Port of London extended to the mouth of the river. They had not had a single case of plague in the administrative county of London, although there had been four or five suspicious cases.

Dr. F. Tidswell (Sydney), in replying to the observations, said that with respect to the part played by fleas, the work at Sydney had been conducted in view of Simond's researches on that subject. The inoculation of Haffkine's prophylactic had been deliberately restricted to persons exposed in the infected area, with a view to securing its aid in restraining the epidemic. Whether or not it had absolutely protected any individual could not be stated. The half-dozen inoculated persons who subsequently acquired plague had all been mild cases and had all recovered, whereas in the ordinary cases the fatality had been about 33% or 34%. It would thus appear that the prophylactic had been of considerable efficacy in minimising the severity of the disease. The Pasteur Institute serum had been used in the treatment of patients. It had not been attended with such markedly beneficial results as were observed with regard to the curative serum for diphtheria. He would not like to pronounce more definitely upon its efficacy, as the cases treated were comparatively few, and perhaps no rigid inference should be drawn from them.

Dr. S. M. Kaka, referring to the anti-plague serum, said it was used in Karachi, but he could not say that the results obtained were any better than from the ordinary treatment in hospitals. Experiments were still going on at Bombay, but he was afraid, even if the serum was shown to produce beneficial results, it was beyond the reach of the poor on account of its prohibitive cost.

A vote of thanks to Mr. Shirley Murphy for presiding concluded the proceedings.

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